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AN ESSAY ON AUSCULTATION AS APPLIED TO CARDIAC DISEASES,

CONTAINING A NEW HYPOTHESIS REGARDING THE SOUNDS OF THE HEART.

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[Communicated for the Boston Medical and Surgical Journal.—Continued from p. 344.]

2. The *morbid murmurs and the purring tremor of the heart and arteries.*

(A) *The murmurs of the heart and arteries.* Besides the two natural sounds of the heart, various other sounds are observed in diseases of this organ, which have received the common generic term *murmurs.* These various murmurs have received different names, from their resemblance to some familiar sounds; as the *bellows—hissing—whistling—musical—whizzing—filing—rasping*—and *sawing-murmurs.*[*] They may all be considered as mere modifications of the same kind of sound, which varies from a smooth rushing noise resembling the blast of a bellows, to a harsh grating sound like that produced by a rasp or a saw.

The mechanism of these murmurs, and the diagnostic indications which they afford, have occasioned much diversity of opinion; and the subject is still involved in no little perplexity.

They are so commonly associated with contractions of the cardiac orifices, that, in the first edition of Laennec's Treatise, they were considered as almost certain diagnostic signs of such contractions. He supposed that the harsh rasping or sawing murmur indicated valvular contraction from rough osseous depositions; while contraction from a smooth, yielding substance, occasioned only a soft bellows-murmur. He had no apprehension of the production of a murmur, except from some obstruction to the natural onward course of the blood; and hence, before the publication of his second edition, he met with so many cases, in which dissections disappointed his diagnosis, that he was led to undervalue the diagnostic indications of these murmurs. In the subsequent editions of his work, he explained this phenomenon, by attributing it to a "*spasmodic contraction of the heart or arteries.* On many occasions," he says, "I have been struck with the complete resemblance of the sound produced by muscular contraction and that of the bellows-sound. In resting the ear upon a pillow, if we contract the masseter muscles, or rather if we contract and relax them alternately, we give rise to sounds precisely like the bellows-sound." (Forbes's Laennec, Ed. 1828, p.

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[*] Dr. Hope, and some other authors, use the term *bellows-murmur* in a generic sense—considering the others as varieties of this. For a generic term, I think that simply *murmur* is preferable, with the prefix *cardiac* or *arterial* to distinguish them, as occurring in the heart or arteries.

569.) He still considered the murmur as a probable diagnostic sign of valvular obstruction—not, however, as at first, attributing the mechanism of the murmur to "the passage of the blood over a rough or rugged surface, but to a spasmodic energy requisite in the *muscular contraction* to overcome the obstacles opposed to it." In short, he attributed the murmur immediately to the motions of the heart or arteries—not to the motions of the blood.

Dr. Williams says, that he is "disposed to think that were we better acquainted with the laws of the production of sound, we might find that it may be excited *by the motion of liquids*, as well as by that of air, in or against solids of a particular form; and that we might find a more satisfactory explanation of the phenomena in question in *the moving mass of blood being thrown into sonorous vibration by some modification in its course.* Such a modification might be produced by thickening or irregularity in one of the valves of the heart, or by spasmodic action of some of the columnæ carneæ; by any obstacle in the cavity of an artery, &c.: and these causes might, as in the analogous case of air, render the passage of the blood sonorous, instead of, as it usually is, silent." (Rational Exposition, p. 62, Philadelphia Edition.)

Dr. Williams adds, "These are but conjectures." The principle advanced by him, however, it is believed, is now almost universally received. Indeed, no fact can be more evident, than that sounds may originate in liquids—being "excited by the motion of liquids," and propagated through the medium of them. Any one in the habit of bathing must have observed, that the motions of the limbs are distinctly audible, to a person immersed in water: and if, under such circumstances, two hard bodies, as two stones, are forcibly struck together, a loud sound, even painful to the ear, is excited.

The principle advanced by Dr. Williams, is adopted by Dr. Hope, whose explanation of the murmur is ingenious, and, in general, satisfactory.

Cardiac murmurs. "Valvular murmurs," says Dr. Hope, "are occasioned by collision of the particles of the blood, when this fluid is, by any cause, thrown into preternatural commotion during its passage through the orifice of a cavity. To offer an experimental exemplification of this—a similar murmur is produced when water is transmitted with sufficient velocity through a tube, in any part of which there exists an internal prominence or contraction of its calibre. The same occurs when the leather pipe of a fire-engine is slightly compressed with a finger; or when similar compression is exercised with the stethoscope on a superficial artery of primary or secondary magnitude."

"Obstructions of a hard, rugged kind, as ossifications, occasion louder murmurs than smooth obstructions, because they more completely break the current of the blood. Murmurs are not, as is often supposed, louder, cæteris paribus, in proportion as the valvular contraction is greater. On the contrary, the loudest murmurs are produced by a moderate contraction, and they become weak when it is extreme. Thus, a rugged osseous concretion, the size of an ordinary pea, in the aortic orifice, I have found to produce the loudest possible murmur; whereas, a contraction of the mitral or tricuspid valve to the size of only two, three, or four lines in diameter, I have frequently known to occasion little or no murmur. Osseous asperity, alone, without contraction, produces considerable murmur. From the above premises, it may be stated as a general principle, that the loudness of a murmur is in proportion, not only to the roughness of the obstacle, but also to the quantity of fluid transmitted through the valve, and put in preternatural commotion by the obstacle. The effect we should naturally expect to be aided by the force and velocity with which the fluid is impelled; and, accordingly, we find that when the ventricle is hypertrophous, or the circulation hurried, the murmur is proportionally louder. A simple illustration of these doctrines may be obtained by employing air instead of fluid, as the sonorous medium. Thus, if air be blown with equal velocity through a large, and a small orifice or tube, the sound is louder from the former. If the velocity be increased, the sound is proportionally augmented, and it is, cæteris paribus, always louder when the tube or orifice is rough or unequal." (Hope's Treatise, pp. 56—58.)

Cardiac murmurs, then, may be caused by a thickening of the valves resulting from inflammation; by a cartilaginous or osseous induration of the valves; or by any circumstance which prevents the free motion of the valves—by warty and other excrescences about the valves or orifices —and, in short, by any obstacle to the natural current of the blood through the orifices.

The situation in which a murmur is most audible, and the time of its occurrence, will commonly indicate the particular orifice which is obstructed. Obstruction of either of the arterial orifices, occasions a murmur at the time the blood is propelled through the orifice by the ventricular contraction—that is, accompanying the impulse and first sound: whereas, obstruction of an auriculo-ventricular orifice occasions a murmur from the rushing of the blood through the orifice during the ventricular diastole—that is, at the time of the second sound.

An important fact, unnoticed by Laennec, Andral and Bertin, has been observed by Dr. James Johnson, Dr. Elliotson, and others, that a *murmur from regurgitation* frequently occurs, in consequence of an imperfect occlusion of the orifices by the valves. In many cases the valves become rigid from cartilaginous or osseous degeneration, or deranged in shape, so as to be obviously incapable of closing the orifices. In such cases, as Dr. Hope says, when the *mitral* or *tricuspid* valves, "not closing accurately, admit of regurgitation, a murmur accompanies the first sound;" and when the *aortal* or *pulmonic* valves, "not closing accurately, admit of regurgitation, a murmur accompanies the sound of the ventricular diastole." In consequence of this fact it is sometimes difficult, if not impossible, to determine with certainty which valve is disordered. For instance, a murmur heard on the left side of the heart, during the ventricular contraction, may be owing either to obstruction of the aortal orifice, or to a patescence of the mitral orifice; so, a murmur heard during the ventricular diastole, may be owing either to a contraction of the mitral orifice, or to a patescence of the aortal orifice. In many such cases there are signs by which the particular valve disordered can be determined; a precise diagnosis, however, is commonly unimportant, the pathologic effect being, in general, the same—that is, a disturbed and labored circulation through the left side of the heart.

Besides those cases in which there is evidently some derangement of the valves or orifices, cardiac murmurs occur in many cases in which dissection shows no organic derangement. I am disposed to believe, however, as will be presently noticed, that in most such cases there is at least some *functional* disorder of the valves.

Arterial murmurs. Murmurs similar to those which originate in the heart, are frequently observed in arteries of a considerable size—caused, as Dr. Hope supposes, by the collision, or friction, of the particles of blood. They are produced by osseous depositions, or any other obstructions, within the arteries; and can be excited, in any considerable superficial artery, by compressing the artery with the end of the stethoscope. According to established principles of hydraulics, it might be expected that any sudden bends or irregularities in the arteries would increase the friction, and cause a retardation, of the blood. It is discovered by hydraulic experiments, also, that an abrupt, bulging expansion of any part of a tube through which water is rapidly impelled, occasions a murmur. A hissing murmur from this cause is observed when the ear is applied over the region of an aneurism—an application of immediate auscultation noticed by many of the older surgeons, Richter, William Hunter, &c., long before the discoveries of Laennec.

A remarkable bellows-murmur, discovered by Kergaradec to be a diagnostic sign of pregnancy, originates from the arteries of the placenta or uterus. This is commonly supposed to be occasioned by the tortuous course of these arteries; perhaps, however, it is to be explained, in part at least, on the same principle as the murmur of aneurisms, from the bulging expansion of these arteries necessarily accompanying the enlargement of the uterus and its appendages, during pregnancy.[*]

(B) *Purring tremor of the heart and arteries.* In some cases the hand applied to the precordial region, or over an artery of considerable size, feels a peculiar vibratory or thrilling sensation, which, from its resemblance to the tremulous motion occasioned by the purring of a cat, and felt by applying the hand to the back of the animal, is commonly termed the *purring tremor* (frémissement cataire)—we meet also with the terms *purring thrill—whizzing tremor, thrill,* or *vibration—vibratory thrill—arterial thrill—thrilling pulse, &c.* This tremor commonly accompanies the murmurs of the heart and arteries—indeed both phenomena are produced by the same causes. Particular motions of the blood within the heart and arteries occasion vibrations, or tremors, perceptible to the hand; and the same vibrations, propagated to the ear, give the sensation of sound—in other words, these tremulous motions of the heart and arteries can be both felt and heard.

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[*] Dr. John D. Fisher, of Boston, has published in the Medical Magazine, Vol. II. p. 144, an account of a "*Cephalic bellows-sound,*" which he has observed, with the ear or the stethoscope applied to the head—occasioned, as he supposes, by "either dropsy of the brain or a congestion of the bloodvessels within it;" the immediate cause of the murmur being the compression of the arteries at the base of the brain. Since reading the interesting observations of Dr. Fisher, I have met with only two cases in which this murmur was observable. In one, a fatal case of hydrocephalus in a child three years old, a considerable quantity of water was found in the fourth ventricle and at the base of the brain. In the other case, that of a child five months old, there were general symptoms of encephalic congestion, with the abatement of which the murmur subsided.

Corvisart, who depended much on the manual examination of the exterior of the chest, considered the purring tremor as an important diagnostic sign of valvular derangement; and Bertin states, that he regards "the vibratory tremor and thrill as infallible signs of contraction of one of the orifices of the heart."

(c) *General Observations.* From the preceding observations it will appear that the various morbid murmurs and the purring tremor of the heart and arteries, are all modifications of one kind of phenomenon, produced, in some way, by the friction, or vibratory collision, of the particles of the blood. The causes of this friction, or vibratory collision, in those cases in which there is an evident obstruction to the current of the blood, or an evident patescence of a cardiac valve, have been sufficiently explained.

There are many cases, however, in which the causes of murmurs and tremors are less obvious. Murmurs and tremors of the heart frequently occur in hypertrophy with dilatation, in carditis and in pericarditis; and they are observed, both in the heart and arteries, in cases of irritative hemorrhage, in cases of reaction after excessive depletion, and in nervous affections attended with an irritable condition of the system, as in hysteria, hypochondriasis, &c. In the most of these cases, Dr. Hope thinks the murmur and tremor are produced by an "irritable state of the nervous system," which causes a "spasmodic abruptness of the heart's contraction," propelling the blood with increased velocity—"a velocity which implies an augmentation of friction, and consequently of arterial vibration."

He remarks, that "the sharp, jerking pulse and beat of the heart of a patient in a state of nervous agitation, is too well known to require demonstration. Sometimes, when the nervous excitement is excessive, a violent throbbing is perceptible over the whole body, and the bellows-murmur and thrill are distinct in every considerable arterial trunk." These occur, he says, "independent of an increase of *real* force of the heart's contraction, as is proved by their existing when the pulse is small and weak, provided it is jerking or sharp." In conclusion, he remarks, "that the *purely inflammatory pulse*, though more or less quick, full, strong, and hard, is not jerking nor attended with bellows-murmur. These distinctions, which, to the inexperienced, may appear refined in description, are perfectly familiar to practical men; and it is of great importance to the young practitioner that he make himself intimately acquainted with them, as such knowledge will not only facilitate his diagnosis, but prevent the unnecessary, and often, in nervous cases, pernicious abstraction of blood for imaginary fever or inflammation."

Few errors in practice are more common and pernicious than those which originate from neglecting these distinctions to which Dr. Hope refers. In cases of atonic, acute rheumatism, and in many *simply irritative* febrile affections, attended with a quick, sharp, jerking pulse, and considerable heat of skin, bleeding is very commonly resorted to—frequently, indeed, with manifest temporary relief of pain, and with a general mitigation of symptoms—but, in such cases, the pain and other symptoms ordinarily return, with redoubled violence, after an interval of from six to twenty-four hours.

As before remarked, *patescence of the cardiac orifices admitting of regurgitation*, has been but recently noticed as a cause of murmurs and tremors; and I am inclined to believe that this cause is much more common than Dr. Hope and other recent authors have supposed.

In *hypertrophy with dilatation*, a murmur frequently accompanies or supersedes the first sound. This is thought by Dr. Hope, "partly attributable to the increased quantity and force of the blood expelled by the ventricular systole," and partly to "the changed form of the ventricle," in consequence of which "the currents of blood reflected from its sides meet in the [arterial] orifice at more obtuse angles, and thus, by their collision, not only give rise to the murmur, but impede each other's passage into the vessel. For the latter reason the pulse is sometimes small and weak, when the impulse of the heart is violent—a paradox with which authors have been much perplexed." It appears to me, however, that the supposition of a regurgitation at the auriculo-ventricular orifice much more rationally accounts for the murmur; while it explains the "paradox" of the small and weak pulse, and, at the same time, the fact (which can hardly be explained by Dr. Hope's hypothesis) that in this affection the murmur frequently entirely supersedes the first sound. Dr. Elliotson, Dr. James Johnson, and others, have observed that, in this affection, the orifices commonly partake of the general enlargement of the heart, so that the valves frequently become insufficient to close them. But, even if no disparity between the orifices and the valves exists, it is obvious that the change in the relative positions of the columnæ carneæ, and the altered direction of the chordæ tendineæ, necessarily accompanying the enlargement of the ventricle, may prevent the perfect occlusion of the valves.

So, in *carditis* and *pericarditis*, there is much reason to believe that the murmur is owing to a disordered condition of the valves or orifices, which commonly partake of the diseased affection of the heart and pericardium. Dr. Elliotson says, "I have never opened a person whom I had seen laboring under chronic pericarditis and affording the bellows-sound constantly, in whom a diseased condition of an opening or passage from the heart did not present itself."

I am disposed to believe, however, that, independent of any defect of the valves, or of any organic disorder of the heart, a murmur from regurgitation in many cases accompanies the first sound, in consequence of simply *a disordered function of the valves*. The occurrence of a bellows-murmur, in cases of a general exsanguious condition of the system, induced either by disease or by excessive depletion, and in cases of nervous affections, as hysteria, hypochondriasis, &c. are adduced by authors to prove that this murmur may occur independent of the action of the valves. But is it certain that, in these cases, the valves perform their office perfectly? Is it certain that all of the columnæ carneæ act perfectly in concert with each other, and with the muscular parietes of the ventricles? In such nervous affections, the muscles of the system generally are frequently subject to irregular spasmodic actions, and the muscular apparatus of the heart is probably not exempt from the general laws of muscular irritability. A slight spasmodic contraction of one of the columnæ carneæ, or a disparity of action among the different columnæ, obviously might prevent the perfect occlusion of the valves, and occasion a regurgitation. It is not improbable, moreover, that, in cases of a general exsanguious condition of the system, a shrinking of the ventricular parietes may occur, in consequence of inanition of the heart, sufficient to occasion a regurgitation, by deranging the relative position of the columnæ carneæ, and relaxing the chordæ tendineæ and valves.

A murmur from regurgitation accompanying the second sound is of less common occurrence—because, the arterial orifices do not so commonly become dilated, in cases of a general enlargement of the heart; and because the arterial valves, being of a more simple mechanism, are less liable to derangement than the auriculo-ventricular valves. Dr. Elliotson describes an interesting case of a murmur from regurgitation, in place of the second sound, in consequence of a rupture of one of the sigmoid valves of the aorta (Lon. Med. Gaz. Vol. VII. p. 793); and several cases are recorded of such murmur occasioned by excrescences about the arterial orifices, or by irregularity or rigidity of the arterial valves.

In conclusion, I would remark, that my hypothesis with regard to the valvular origin of the natural sounds of the heart, derives no little confirmation from the fact, that *when murmurs occur in consequence of a derangement of the cardiac valves, the natural sounds which I suppose to be produced by those valves are commonly modified, and sometimes entirely replaced, by the morbid murmur.* This fact is so generally conceded by the authors who have had various hypotheses to support, that it would be superfluous to refer to authorities. Laennec, in speaking of the murmur, says, "and when present, it entirely replaces the natural sound of the ventricle, auricle, or artery;" and Dr. Haycraft observes (Lond. Med. Gaz. Vol. VIII.), "I have in vain sought for cases in which considerable structural narrowing of the mitral passage, such as would occasion a bellows-sound, was accompanied by a completely natural first sound of the left side."

[To be continued.]

PURITY OF CHARACTER IN A PHYSICIAN.

To the Editor of the Boston Medical and Surgical Journal.

Sir,—In a late number of the Journal, I have been gratified in finding an extract, relating to the *danger attending the solitary examination of female patients.* It is a subject well worth the attention of the younger part of the profession. Previous to my beginning practice, I studied Denman very thoroughly, and one of his remarks made such an impression, that I have not to this day forgotten it. He lays it down, as an indispensable rule, **never** to propose an examination (*per vaginam*), except in the presence of witnesses. I have always endeavored strictly to conform to the spirit of this rule, and consequently have never met with any difficulty. The same caution is applicable to every other case, where similar delicacy is required. The only instances, in which I have allowed myself to deviate from the rule, have been in syphilis, where I have been privately consulted. These are often embarrassing, on this very account.

It is very rare for a physician to be involved in any difficulty, unless he, directly or indirectly, violates Denman's rule. Most female patients will instantly perceive whether a physician attends to his business, as a matter of business, or whether he does it as an excuse to gratify a voluptuous curiosity. Nothing has a tendency to sink a practitioner, in the estimation of all decent people, more than the latter propensity, which is easily detected, though he may not be guilty of a single overt act which can be stated in words, so as to make the fact certain to a third person. Professional men have extra pay for their services; and one reason is, they have extra confidence placed in them. They cannot, therefore, be too cautious in all their conduct, lest they lessen that confidence. I have always observed that women of the poorest reputation will generally pay the greatest respect to a physician, and never dare to throw out any allurements to him, provided he only respects himself, and preserves a proper dignity.

In the case complained of in the Journal, which is the occasion of this letter, it seems that the physician did deviate from the rule; he must, therefore, have subjected himself necessarily—except perhaps in a case of syphilis—to suspicion, and have placed himself, in some degree, in the power of a woman, who might be disposed to injure him. Whether her complaint is well founded or not, he has his own indiscretion more to blame than anything else, if we may judge of the story as it is related. In a celebrated case near New York, a few years since, the physician deservedly suffered for his great indiscretion and imprudence, though it is very possible that there was not the least impurity in his intentions. Such an inquisitiveness as he acknowledged, when no witness is present, is ever to be reprobated. If tolerated, it would soon produce a lax state of morals.

We are all familiar with the old Norman French motto, *honi soit qui mal y pense*. It will apply peculiarly to medical and physiological investigations. All kinds of information are proper, when obtained with proper views, for useful purposes. But there is nothing more disgusting, or a greater evidence of a corrupt taste, and a striking defect of moral principle, than a disposition to make anatomy, physiology, therapeutics, and obstetrics, subservient to a voluptuous imagination. There is in fact a kind of treachery about it—while science is the ostensible subject, to make our professional knowledge subservient to the passions and appetites. Many French writers are most grossly culpable in this respect, and habitually intersperse obscenity in their scientific works. Many of their modes of practice in several female complaints, are no better. I am sorry to see this French taste attempted to be introduced into this country, and presented for the admiration and imitation of our young men. Maygrier's work on obstetrics is of this kind. Many of the plates are entirely unnecessary in a professional work. The book contains nothing new, that is useful. Smellie's and Hunter's plates, and those in Parr's Dictionary, as well as many others, were already amply sufficient, without the aid of this new and sensual compilation. Besides, upon scientific principles, they are positively wrong. Anatomical engravings and drawings should be made to assist the understanding, and not to be calculated to excite the imagination, and inflame the passions. In other words, in general, they ought to be *maps*, rather than *pictures*.

It is very much to be wished that our professors would occasionally give lectures upon the prudential and moral part of the practice of physic. This is capable of being reduced to rules, and of being taught, as much as any of the other duties of the profession. Strict morals become a physician, and are as necessary to his extensive usefulness as to any other man in society. He has, at times, unbounded confidence placed in him, and it is extremely important that it should not be in the least weakened by the suspicion or the slightest appearance of evil.

There is a strong tendency to degeneracy in man, and every institution in the world needs a constant and vigilant oversight, or abuses will infallibly gain admission. It is very necessary that the imaginations of our youth, in the most effervescent period of life, should not be polluted, under the pretence of pursuing philosophical and professional studies. Temptations are already too numerous, and they will be increased in a tenfold ratio if science becomes subservient to voluptuousness. A late traveller, with much apparent justice, thinks that the paintings and statuary of the Louvre, as well as innumerable exhibitions of the kind in every part of the city, are alone sufficient to account for a great proportion of the corruption of Paris. There, as already remarked, science very frequently takes part in the general pollution. Such abuses are very rare in England, and, I believe, never to be found in the best medical and philosophical writers of that country. It is confidently hoped that the attempts to introduce such a false and corrupt taste in the medicine of our country, may be nipped in the bud, and not be suffered to pollute the imagination of the rising generation. *Facilis est descensus !*

Senex.

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DR. BEAUMONT'S WORK AND EXPERIMENTS ON DIGESTION.

It will be recollected by all those who have read our Journal published on the 18th September last, that Prof. Sewall, of Washington, related the case of a boy, on whom Dr. Beaumont, of the U. S. Army, was pursuing a course of the most interesting and important experiments. The man, now about 26 years of age, received a wound in the left side when eighteen, that left an aperture into the stomach through which articles might be directly introduced either for food, physic, or experiment. He fell, at the time of the wound, under the surgical care of Dr. B., to whose pro-

fessional skill the young man probably owes his recovery, and who has since supported him and his family for the purpose of pursuing a series of physiological experiments. These experiments, with Dr. Beaumont's reasonings on them, and an account of the case, are now before the public, in a large 8vo. of nearly 300 pages; and a work it is of rare interest and value—a work that will doubtless find its way to the table of every medical practitioner in this country, and be republished abroad, and translated into the various languages of continental Europe.

With this opinion of the interest that will be taken in these experiments—an interest that will lead each of our readers to peruse the work itself—we do not feel it necessary to enter into their history, as we should do if they came in a less accessible form; but shall merely and briefly notice, as permanent contributions to our knowledge of the digestive process, so far as this process takes place in the stomach, some of those conclusions at which the observations of Dr. B. have enabled him to arrive, which are different from, or additional to, those of other physiologists whose works form the authorities of the day. Should we, in this cursory notice, find it not easy to adopt all the reasonings of Dr. B., we would still award him all the praise of having improved, with great industry and judgment, an opportunity for physiological observation that may never again occur to any medical man, and of having recorded with clearness and precision the numerous facts that give such interest and value to his work.

In the first place, it has been the common opinion among physiologists that the introduction of saliva into the stomach promotes the digestion of food by aiding the solvent power of the gastric juice, or facilitating its solution when arrived within the cavity of the organ; and some have even maintained that the saliva is the *principal* solvent of the aliments. Dr. B. came to a different conclusion. He found that when substances were introduced directly into the stomach, the digestive process went on with the same regularity, and the chyme formed had the same qualities, sensible and chemical, as when the food passed through the mouth in the usual manner. It would even appear from some of the experiments on *artificial* digestion, which were made for the purpose of comparison, that the mixture of saliva with the gastric juice rather retarded its solvent action. Dr. B. concludes, therefore, that the saliva is useful not as a solvent to the food when in the stomach, but, by mixing with it, during the process of mastication, to soften and lubricate it, and render it fit to be acted on by the organs of deglutition. Dry food, he remarks, cannot be swallowed until it has received the admixture of a fluid; but whether this be saliva or some other liquid, is not, he conceives, a matter of very great consequence. Water will answer the purpose nearly as well, though the mucilaginous qualities of the secretion may tend to give it some degree of preference.

To these views of Dr. B. it may perhaps be objected, and not without some show of reason, that if the only use of the saliva were to soften and break down the food, it would be sufficient to swallow food already reduced to the form of a pulp or in a liquid state, in order to ensure its easy and perfect digestion; whereas in practice this is not found to be the case. As respects the proof drawn from the appearance of food introduced directly into the stomach, it may be that the very stimulus of this food on the surface of the organ causes a flow of the saliva to the stomach, and that in this way it is enabled to perform its usual part in promoting the digestive process; or the stimulus of food thus introduced may perhaps excite an action of the gastric vessels greater than that which would have resulted from the contact of the same article received in the shape of pulp from the admixture of saliva, and an increased quantity of gastric fluid would thus be thrown out, and the aliment be as thoroughly and speedily chymified as if regularly masticated: and yet it would not follow, we conceive, that the saliva does not usually *facilitate* chymification—render this process *easier*, inasmuch as it calls for less action on the part of the stomach ;—for, among the most undoubted facts stated by Dr. B. is this, that "the gastric juice does not accumulate in the cavity of the stomach, until alimentary matter be received, and excites its vessels to discharge their contents, for the immediate purpose of digestion."

These remarks we make, not so much to express any dissent from the conclusions of Dr. B., as to show that there may be circumstances that modify, in an essential manner, conclusions that may at first appear exceedingly manifest. We confess, however, that Dr. Beaumont's view of this subject does appear to us still to want something of a rigid demonstration.

The next opinion adopted by existing authorities, which Dr. B. feels himself called on to combat, is, that digestion does not commence immediately as the food is received into the stomach, the space of about an hour usually elapsing between the time of its introduction into that organ and the commencement of the action of the gastric juice upon it. This doctrine has long been held by physiologists, and is particularly stated by Dr. Paris. The experiments of Dr. Beaumont appear fully to justify his dissent from this long-established theory, though there are some of his arguments on this topic that will hardly bear a very close examination.

A third subject on which Dr. B. has been led to impugn the correctness of existing opinions, and one more within the compass of ordinary observation, is, the necessity of rest after taking food. Rest after a meal has heretofore been deemed favorable to its full and easy digestion. Dr. B. has found it otherwise. Now the truth as respects each individual is indeed matter of individual experience ; and it is well known that a large portion of mankind do actually labor after eating, as well as during fast, without incurring any evil consequences : so that whatever the natural demands of the system may be, they seem to be wholly controlled, in this instance, by the force of habit, which here becomes a second nature. But where the natural wants of the digestive organs are not modified by established habits, there does doubtless exist a uniform difference between the action of those organs during exercise and rest, and we have always regarded the evidence adduced in favor of the latter as insufficient to justify the very general belief of its good influence on the digestive function. Beasts of prey and savages, it is true, sleep after eating ; but it is also true that the quantity of food taken by either is far beyond the demand of an appetite which is satisfied at the usual intervals of meals as they occur in civilized life ; and since examining the experiments of Dr. B. we are more than ever inclined to the belief, that, in a healthy state of the system, and when repletion is not excessive, moderate exercise is more favorable to digestion than perfect rest ; and the fact that those who remain at rest after eating become drowsy, or sleep, is rather an argument in favor of this supposition than otherwise. The tables given by Dr. B. show very clearly that about 1-4 or 1-5 more time is required to digest the same article when the body is at rest, than is required when it is in motion.

Dr. Philip and many other physiologists have thought that when food has been taken at different but not distant times, the new is never mixed with the old food in the stomach. Dr. B. has found this opinion to be erroneous, an intimate admixture of the whole contents taking place without delay.

Bile has been supposed by some to be always present in a healthy stomach, and necessary to perfect chymification. This is not so, as shown by the frequent observations of Dr. B. That fluid, however, readily flows into the stomach under certain circumstances, and this phenomenon is regarded by Dr. B. as most generally, if not always, morbid When an oily substance has been introduced into this cavity, a portion of bile was seen to have found its way through the pyloric orifice ; it also flowed in when the pylorus was irritated with the end of an elastic tube or the bulb of the thermometer, and *when the man was in a paroxysm of anger*.

We find ourselves pursuing this enticing subject further than was intended, and will only add the following extract from Dr. Beaumont's work, giving an account of the *motion* of the food in the stomach.

"The ordinary course and direction of the revolutions of the food, are first, after passing the œsophageal ring, from right to left, along the small arch ; thence, through the large curvature, from left to right. The bolus, as it enters the cardia, turns to the left ; passes the aperture ; descends into the splenic extremity; and follows the great curvature towards the pyloric end. It then returns, in the course of the smaller curvature, makes its appearance again at the aperture, in its descent into the great curvature, to perform similar revolutions.

Such I have ascertained to be the revolutions of the contents of the stomach, from being able to identify particular portions of food, and from the fact that the bulb of the thermometer, which has been frequently introduced during chymification, invariably indicates the same movements. These revolutions are completed in from one to three minutes. They are probably induced, in a great measure, by the circular or transverse muscles of the stomach, as indicated by the spiral motion of the stem of the thermometer, both in descending to the pyloric portion, and ascending to the splenic. These motions are slower at first than after chymification has considerably advanced.

While these revolutions of the contents of the stomach are progressing, the trituration or agitation is also going on. There is a perfect admixture of the whole ingestæ, during the period of alimentation and chymification. There is nothing of the distinct lines of separation between old and new food, and peculiar central or peripheral situation of crude, as distinguished from chymified aliment, said to have been observed by Philip, Magendie and others, in their experiments on dogs and rabbits, to be seen in the human stomach ; at least in that of the subject of these experiments. The whole contents of the stomach, until chymification be nearly complete, exhibit a heterogeneous mass of solids and fluids ; hard and soft ; coarse and fine ; crude and chymified ; all intimately mixed, and circulating promiscuously through the gastric cavity, like the mixed contents of a closed vessel, gently agitated, or turned in the hand."

It would be highly gratifying to the profession—and doubtless the result would prove equally so to Dr. Beaumont—were the opportunity presented to other physiologists to repeat the experiments already made on this young man, and perhaps to modify and extend them.

VALUE OF CHLORINE INHALATIONS IN PULMONARY CONSUMPTION.

Much has been written on this subject, and we seize with avidity on the testimony respecting it, of such a man as Dr. Stokes of Dublin. We therefore present below an extract of one of his recent lectures at the Meath Hospital, which contains the sum of his observations of the effect of this mode of treatment.

"We have two cases," says he, "at present in the hospital, which have been for some time under the use of chlorine inhalations. The patients are, as you are aware, laboring under phthisis; and I need not say, that in subjecting them to this mode of treatment, I did not set out with the expectation of their deriving any considerable and decided advantage, for the cases were at the period of their admission essentially hopeless. They had all the symptoms which, as I mentioned to you on a former occasion, pointed out the incurability of phthisis. In the first place, there was in both not only symptoms of phthisis, but also a confirmed hectic, a circumstance which is indicative of much more extensive disease than the occurrence of mere suppuration in the lungs. This is a point to which I would direct your attention. In pulmonary diseases we often find that hectic symptoms are connected *more with the extent and incurability of the disease than the mere presence of matter in the lungs*, and hence it is that we frequently see hectic coming on before the actual suppuration of tubercles. On the other hand, you will see numerous instances of tubercular cavities without hectic. We often meet with patients who have symptoms of abscess in the lungs, with intense bronchitis and other very urgent symptoms, and we frequently find that by treatment calculated to subdue the bronchitis we are able to put a stop to the hectic, although the suppuration still goes on. You will be called to attend persons in consumption, who are laboring under hectic fever, and on inquiry you may find that they are using wine and animal food; put them on milk diet, and you will often observe an extraordinary improvement in the febrile symptoms, though there is no amendment, so far as suppuration is concerned.

In the other case of phthisis, where chlorine inhalations have been used, there is a disease of the larynx, and in both we have evident symptoms of derangement of the digestive organs, so that every circumstance tends to render them cases in which very little can be accomplished. I have put them, however, on chlorine inhalations, more for the purpose of observing its effect on the system than from any hope of deriving advantage from its employment. You are aware that this remedy has been recommended by many practitioners, and by none more particularly than Cotteran, who published a memoir on this subject in the 'Archives Generales de Medecine.' The reason why I direct your attention to this subject is, that there are statements put forth in that memoir which I have not been able to verify. Cotteran states that he has treated cases of phthisis successfully by means of the chlorine inhalation, and he professes himself to be a stethoscopist. If a man who is not a stethoscopist states that he has cured phthisis, the question is then less as to the curability of the disease than the correctness of the diagnosis; but when a stethoscopist tells you that he has cured cases in which there have been hectic symptoms with cavernous respiration, and dulness of sound on percussion beneath the clavicles, it is certain the case is altered. Now he states that he has treated several cases successfully, and brings forward instances of decided cure. I shall make no comment on this, but shall only mention that in this hospital we have treated a great many cases with chlorine inhalation, and I regret to say the results have not been such as to enable us to recommend its use. In a few cases it has effected some slight good, in several we have done positive mischief, and in the majority of instances we have done neither good nor harm. You remember I mentioned to you on a former occasion that the essence of this plan of treatment is simply this, an attempt to cure chronic inflammatory disease by stimulation. The principle appears to be just the same as that by which chronic mucous discharge from the urethra is cured with balsam copaiba, or chronic diarrhœa is cured with turpentine. Besides, you are to bear in mind that you always run a great risk in those cases where you have to deal with an extensive organ such as the lung. In phthisis the expectoration is frequently very copious; there is an immense secretion going on, which may in some degree be looked on as a relief to the inflammatory action. If you check this secretion, you will find that the patient's sufferings are intense, and that matters are in a more unfavorable condition than before. This principle, gentlemen, applies to every case of direct stimulation. When the chlorine inhalation had a tendency to diminish expectoration, we found that it was followed by very unpleasant symptoms, such as those which occur when you suddenly check a chronic diarrhœa, or any other continued discharge. We observed that where the spitting was suddenly diminished, the result was intense dyspnœa, fever, pains in the chest, and the stethoscopic signs of pneumonia in the lower part of the lung, which had been previously free from disease. In two or three cases the symptoms of pneumonia were so violent that we were obliged to have recourse to venesection. We also noticed that when the inhalations were discontinued, the patients in a few days returned to their former condition.

There is another circumstance connected with this practice which is worthy of remembrance. In many instances it appeared to produce a kind of revulsion from the chest to the abdominal viscera; the cough and expectoration were lessened, but diarrhœa or vomiting came on, and the patients seemed to have gastritis. The reverse of this has often been observed, as in cases of long-continued diarrhœa, in which by treatment with astringents and stimulants, the discharge has been suddenly checked. We then may see a violent chest affection come on, and a most copious secretion established from the bronchial mucous membrane. Another curious fact is, that the inhalation very frequently produced a state of stupor, the patients appearing to be as it were under the influence of narcotics. I have seen persons fall asleep with the pipe of the inhaling instrument in their mouths. I do not know, however, how far we are to look on this agent as a narcotic, but it certainly produced effects very similar to those which arise from the use of soporific medicines.

This, gentlemen, is the sum of our observations on the use of chlorine inhalation in phthisis. Most of the cases in which it was employed were unfavorable, and there was, for the most part, distinct evidence of disease of the digestive system. If we could have tubercular abscess without disease of the digestive system, or a scrofulous habit, it might do very well. On the other hand, we have derived remarkable benefit from the use of chlorine in a case of gangrenous abscess of the lung, which was in the hospital some time since. The patient was an unfortunate man who in a state of beastly intoxication rolled out of bed quite naked, and lay all night on a damp floor. He awoke in the morning extremely ill, with severe pain in the side he was lying on; fever set in rapidly, his breathing became very much affected, and he began to cough up a quantity of dark fœtid matter. He was admitted into the hospital with the stethoscopic signs of a cavity in the left lung, severe hectic, breath and expectoration extremely offensive. He was ordered to have wine, nourishing diet, and chlorine inhalations. After the lapse of twenty-four hours there was considerable amendment, and after forty-eight the fœtor was greatly diminished, and on the third day the smell ceased to be offensive to persons in the ward. In order to ascertain if this was the consequence of using the chlorine, we ordered it to be omitted for a day or two, and found that the fœtor again returned, but after recurring to its employment the breath improved a second time. On this plan of treatment we kept him for some days, when the stench of the expectoration and breath having entirely ceased, we omitted it altogether. All the gangrenous symptoms returned again, and a third time were removed by the chlorine. It was the internal use of the chloride of lime we employed on this occasion; latterly I have been trying the chloride of soda. It has effected a most decided good in the case, and indeed if we derived no other advantage from its employment beyond that of correcting fœtid breath and offensive expectoration, it is certainly a very material point, for there is no stench in nature more disgusting than that which attends gangrene of the lung, and I am confident a great deal of good was done by correcting this fœtor, and preventing the absorption of putrid matter. I would advise you, therefore, in every case of mortification of the lungs, to direct your patient to use a good, nutritious diet, and make him constantly inhale an atmosphere of chlorine, by keeping his bed sprinkled with a solution of the chloride of lime, and having a vessel containing the solution beneath. You should also take the trouble of ascertaining how the inhaler is used, for some persons draw the chlorine into their mouths only as they would tobacco smoke, and do not let any of it pass into the lungs; it is necessary to show them how to use it, and make them understand that the entrance of the chlorine vapor into the chest is indispensable."

ALCOHOLIC LOTION IN THE TREATMENT OF PULMONARY CONSUMPTION.

It is with great satisfaction we here record the more favorable testimony of Dr. Marshall Hall, to the curative efficacy of another remedy, promising more in this disease than any inhalation of which we have any knowledge.

"I now pass on," says Dr. Hall, "to a more pleasing occupation—viz. to that of detailing the advantages of a remedy in phthisis. This remedy is that to which I have already alluded—the alcoholic lotion. One part of alcohol is mixed with five of water, and applied by means of six folds of linen, three inches in breadth, stretched tightly across the upper part of the thorax; the patient's skin being guarded all over with one fold of flannel. I have this day received the most gratifying tidings of an interesting patient whom I some time ago visited, with Mr. Gardner, at Marlborough. Hæmoptysis and puriform expectoration, with every fearful symptom, have subsided. In *many* other cases of undoubted phthisis the symptoms have been checked—the comfort, the strength, and even the flesh, restored—by this means. One patient called this lotion his 'breast-plate.' Some have worn it for years: a proof at once of their opinion of its efficacy, and of the tendency in this treacherous disease to return. Indeed, although I by no means wish to insinuate that the alcoholic lotion can cure a disease generally incurable (and I would not be again misunderstood—misrepresented), yet I can truly say that this remedy has, in very many instances, conferred a benefit for which I scarcely dared to hope—checking the course of the disease, and modifying, at least, the immediate *prognosis*.

It is but right to state that this remedy has been conjoined with a mild animal diet, regulated bowels, early hours, flannel over the surface, sponging with salt and water, and free exposure to the open air, when the wind has not been northerly or easterly; and that to these have been added, if possible, constant journeying, or sailing, and change of air."

Variola.—The smallpox and the varioloid are now existing—we should not be justified perhaps in using the term prevailing—at Philadelphia.

Medical Institute of Georgia.—The Legislature of Georgia has liberally appropriated $10,000 for the Medical Institute of that State.

We acknowledge the receipt of the valuable treatise on Empyema, in continuation of what has already been published in this Journal. We have thought it best to delay the publication of it till the commencement of our next volume, and hope ere then to receive the remainder of the manuscript.

Subscribers to the Medical Journal who are in the habit occasionally of writing for its pages, are entitled, as they have already been informed, to twelve copies of the numbers containing their communications. This notice is repeated in order further to inform them that these copies, or a part of them, will, if desired, be forwarded from the Journal office to such friends of theirs as they may designate, who are not subscribers.

Whole number of deaths in Boston for the week ending January 11, 30. Males, 17—Females, 13. Of lung fever, 2—typhous fever, 3—dropsy on the brain, 1—consumption, 5—dropsy, 1—fits, 1—stoppage in the bowels, 1—croup, 2—scarlet fever, 2—disease of the heart, 1—jaundice, 1—infantile, 2—throat distemper, 1—cancer, 1—affection of brain, 2—palsy, 1—teething, 1. Stillborn, 1.

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